But not only the degrees of Heat, or points in the ſcale of temperature, at which the forms of different bodies are changed, are various; but the *extent of the variation of temperature* under which a ſubſtance can perſevere, or continue to maintain its form in its *middle ſtate*—that of *fluidity*—or rather *liquidity*,—is very different in different bodies: and this laſt circumſtance has a wonderful effect in increaſing the variety of the compoſitions and decompoſitions which are continually taking place in the various operations of nature on the ſurface of the globe.

Another circumſtance, not leſs prolific in events, is the union which takes place between bodies of *different kinds*; and thoſe moſt important changes in regard to the degrees of Heat which the bodies ſo united can ſupport without having their forms changed, which are found to reſult from ſuch union.

When, to the eſtabliſhed laws which have been diſcovered in the operations of nature in the change of form in ſubſtances that appear to us to be *ſimple*, we add thoſe which have been found to obtain in the changes of form of bodies that are known to be *compounded*, we ſhall perhaps be able to conceive ſome more diſtinct ideas with regard to the nature of thoſe mechanical operations which take place in chemical proceſſes.—I call them *mechanical*,—for mechanical they muſt of neceſſity be, according to the moſt rigid interpretation of that expreſſion.

But the hypotheſis of the exiſtence of *intenſe Heat* in the midſt of cold liquids is ſo new, and ſeems to be ſo contrary to the reſult of all our experience and obſervation, that I feel it to be neceſſary to take ſome pains to illuſtrate the matter.

And firſt, we muſt not expect always to find traces remaining of the exiſtence of intenſe Heat, even where there are the ſtrongeſt reaſons to think it has actually exiſted; for as often as Heat is diſperſed, or carried off, before it has had time to produce any changes of form, or chemical changes or combinations in the bodies to which it is communicated, it leaves no marks behind it.

Fire-arms are often found to miſs fire, even when many live ſparks from the flint and ſteel actually fall into the pan among the priming; but nobody, ſurely, will pretend that the ſmall particles of *red-hot iron* which fall among the grains of the gunpowder, and cool in contact with them, are not intenſely hot;—incomparably more ſo than would be neceſſary to inflame the powder were their Heat of ſufficient *duration* to produce that effect. Had theſe ſmall ſparks been inviſible, it is highly probable that their exiſtence would never have been ſuſpected, and that the fact which they prove would not have been believed.

That gunpowder may be inflamed, it is neceſſary that the ſulphur which conſtitutes one of its component parts ſhould be firſt *melted*, and then *boiled;* for it is the vapour of boiling ſulphur which always takes fire when gunpowder is kindled.

Were melted ſulphur a conductor of Heat, there is reaſon to think that gunpowder would be very far from being ſo inflammable as we find it to be.

As thoſe who have not been much accuſtomed to meditate on the ſubject under conſideration may find ſome difficulty in conceiving how it is poſſible for intenſe Heat to be *excited* in, or to *exiſt* in the midſt of a maſs of any cold liquid, as of water for inſtance, without immediately producing viſible effects, I feel it to be my duty to put that matter in the cleareſt light poſſible, and to ſhow that what I have conſidered as being *probable* is moſt undoubtedly very far from being impoſſible.

The beſt method of proceeding in inquiries of this kind, where the principal object is to diſcover whether a ſuppoſed event, which, from its nature, cannot fall under the cognizance of our ſenſes, is, or is not poſſible, ſeems to me to be, to begin by ſuppoſing the event to have actually taken place, and then to trace its neceſſary conſequences, and compare them with thoſe appearances which are actually found to take place.

Adopting this method, we will ſuppoſe a quantity of pure water, at the mean temperature of the atmoſphere in England, that of 55° F., to be put into a clean and very tranſparent glaſs tumbler, placed in a window and expoſed to the direct rays of the ſun. If the glaſs and the water are both *perfectly tranſparent*, it is evident that no Heat will be generated in either of them by the ſun's light.

If now a ſmall particle of any opaque ſolid body be ſuſpended in the midſt of the water in the tumbler; thoſe rays of light, which, impinging againſt it, are abſorbed by it, muſt neceſſarily generate

Heat in the very moment when they are ſtopped. This is an incontrovertible fact, which nobody will diſpute.

In order to render this imaginary Experiment more intereſting, we will ſuppoſe the ſolid body put into the water to be a ſmall particle of yellow amber; and that its ſpecific gravity is ſo exactly equal to that of the water that it has no tendency to move in it, either upwards or downwards, and conſequently will remain in the ſituation where it is placed, without being ſuſpended; and we will ſuppoſe farther, that this ſolid particle of amber is nearly globular, and $\frac{1}{1300}$ of an inch in diameter, which is juſt equal to the diameter of a ſingle thread of ſilk, as ſpun by the worm; and is probably one of the ſmalleſt objects that is perceptible by the human eye, unaſſiſted by art.

As it is evident that Heat muſt be generated, or excited, in this ſmall particle of amber, by the light it ſtops or abſorbs, the points which remain to be diſcuſſed are, therefore, what *its intenſity* is at the moment of its exiſtence? and what are the effects which it ought to produce in conſequence of that intenſity?

The reaſons have already been mentioned which render it probable that when Heat is generated by the rays of light its intenſity, *where it is generated*—and before it has been diminiſhed in conſequence of its diſperſion, is always the ſame: and taking it for granted that this is the caſe in fact, we will endeavour to trace the operations of that Heat,—

extreme in its intenſity, or degree, but ſmall in regard to its quantity, or to the ſpace it occupies,—which is generated in the particle of amber in the Experiment under conſideration.

As this Heat muſt firſt exiſt where it is generated, it is evident that it muſt exiſt at the ſurface of the particle of amber; and as all ſolid bodies are, in a greater or leſs degree, conductors of Heat, a part of this Heat will penetrate the ſubſtance of the ſolid particle, while another part of it will be carried off by the cold particles of water in contact with the ſurface thus heated by the light.

It remains therefore to be determined what the effects are which this Heat ſo abſorbed, on the one hand, by the ſolid particle of amber, and communicated to the water on the other, ought neceſſarily to produce. And firſt, if the diſperſion of the Heat by both theſe means ſhould be ſufficiently rapid to prevent its accumulation to ſuch a degree as to melt the amber, it is evident that no viſible effects by which its exiſtence could be diſcovered would be produced in that ſubſtance; and this event—(the fuſion of the amber)—will depend on three circumſtances; namely, Firſt, on the temperature at which amber melts;—Secondly, on the facility with which Heat expands and is diſperſed in a ſolid maſs of that ſubſtance, or on its conducting power;—and Thirdly, on the rapidity with which the Heat generated at the ſurface of the amber is carried off by the cold Fluid in which it is immerſed.

Though I do not think there would be any reaſon for ſurpriſe, even admitting the exiſtence of the ſuppoſed intenſe Heat, ſhould the amber be found not to be melted under the circumſtances deſcribed; yet, it appears to me to be extremely probable, that, if amber, in a very fine powder, were mixed with any tranſparent oil, capable of ſupporting a great degree of Heat without being reduced to vapour, and expoſed in it to the direct rays of a very bright ſun, the amber would melt, and be diſſolved; though perhaps very ſlowly.

But if amber does not melt when expoſed in water to the action of the ſun's beams, and conſequently ſuffers no viſible change by which the exiſtence of the Heat ſuppoſed to be generated at its ſurface by the light can be detected, ought not this Heat, were it in fact as intenſe as it is ſuppoſed to be, to produce ſome viſible effects in the water, by which its exiſtence would neceſſarily be diſcovered?

To reſolve this doubt, we muſt inquire what viſible effects it would be poſſible for the Heat in queſtion to produce in the water. Now if we ſuppoſe the water not to be decompoſed by this Heat, which, as no chemical change is ſuppoſed to take place in the amber, cannot happen, the only effect this Heat can poſſibly produce on the water is an increaſe of its temperature, which increaſe muſt, however, be much too ſmall to be detected, either by the feeling, or by the thermometer.

It might perhaps be expected that *ſteam* would be found at the heated ſurface of the particle of amber, and become viſible; but when we conſider the matter for a moment, we ſhall ſee that it is quite impoſſible that ſuch an event ſhould happen; for even on the ſuppoſition (which however is far from being probable)—that the ſame individual particles of water which come into contact with the hot ſurface of the amber ſhould remain in contact with it till their temperatures ſhould gradually be raiſed to that point at which water is changed to ſteam; yet, from the extreme rapidity with which ſteam condenſes when in contact with cold water, it is evident that it could not exiſt an inſtant under the circumſtances here ſuppoſed. Indeed we have direct proofs that ſteam cannot exiſt under ſuch circumſtances, by what is found to happen when large maſſes of iron, or ſteel, raiſed to a moſt intenſe heat, in a blaſt furnace, are ſuddenly plunged into cold water, by ſmiths, in tempering edge-tools; for theſe maſſes of red-hot metal may be diſtinctly ſeen to be in actual contact with the cold water; and did not a part of the water, which is decompoſed by the hot iron, make its eſcape in the form of inflammable air, it is not probable that there would be any viſible appearance from which the formation of ſteam could be ſuſpected.

Hence we ſee the poſſibility of the exiſtence of *intenſe Heat* in the midſt of a maſs of cold water, or of any other tranſparent liquid, without producing any viſible effects; or leaving behind it any traces by which its exiſtence could be ſuſpected.

Let us now conſider a caſe in which this intenſe Heat, though perfectly imperceptible on account of the extreme minuteneſs of the particles of matter in which it exiſts, is capable nevertheleſs of producing very viſible effects. Let us ſuppoſe a ſolution of nitro-muriate of gold, in water, to be expoſed to the action of the ſun's rays. If this ſolution were *perfectly* tranſparent, no Heat could poſſibly be generated in it by light; but as it is not ſo, Heat, in the higheſt degree of intenſity, muſt neceſſarily be generated by thoſe opaque particles (of the oxyd of gold) by which it is ſtopped. Now as gold is a very heavy ſubſtance, it is evident that it muſt be reduced to extremely ſmall particles in order that, when changed to an oxyd by its union with oxygen, it may be diſſolved in and continue ſuſpended in water; and it is clear that the ſmaller any inſulated particle of matter is, at the ſurface of which Heat is generated in conſequence of the abſorption of light, the more ſuddenly muſt the Heat ſo generated be diſperſed through the whole ſubſtance of the particle, and the more equally and more intenſely muſt that particle be heated: from hence it appears evidently, that, if the particles of the oxyd diſperſed about in the water are but *ſmall enough*, the Heat generated in them by the ſun's rays will be ſufficient to expel the oxygen united to the gold, and revive that metal.

There is one very obvious objection, that will doubtleſs be made to this concluſion, which, however, may eaſily be removed. The particle of the metallic oxyd which is ſuppoſed to be heated, is in contact with the water; how does it happen that a great a great part of this Heat does not immediately paſs off into that cold Fluid? I might anſwer, becauſe both water and ſteam are non-conductors of Heat; —and might adduce in ſupport of this reaſon the well known fact, that a drop of water dropped on a piece of iron, heated to moſt intenſe white Heat, will remain ſome time on the iron without being evaporated, even conſiderably longer than if the iron were much leſs hot;—but a circumſtance attending the beautiful Experiment in which iron is burned in oxygen gas, affords a more direct proof of the fact in queſtion.

As this Experiment is commonly made, the iron, which is a piece of ſmall wire, a few inches long, is introduced into a bottle, with a narrow neck, which contains the oxygen gas; the wire being fixed in its place, by cauſing its upper end to paſs through a cork ſtopple, which is fitted to the mouth of the bottle. The lower end of the wire is pointed; and it is ſet on fire by being firſt heated in the flame of a candle, and then plunged ſuddenly, while red-hot, into the bottle. The combuſtion begins the moment the end of the wire enters the oxygen gas; and the metal continues to burn with the utmoſt violence, and with a copious emiſſion of intenſe white light, till the wire, or till all the gas is conſumed, affording one of the moſt brilliant and moſt intereſting ſights that can be imagined.

The product of this combuſtion is the oxygenation of the iron; and this metallic oxyd, in a ſtate of fuſion, and heated to the moſt intenſe white Heat, falls to the bottom of the bottle in globules of different ſizes.

To protect the glaſs againſt theſe drops of calx of iron in fuſion, it is uſual to leave a quantity of cold water in the bottle, enough, for inſtance, to cover its bottom to the height of about an inch: but I have frequently ſeen numbers of theſe globules, much ſmaller than peas, which have not only deſcended *red-hot* through the water; but have remained red-hot at the bottom of the bottle, ſurrounded by the water, at leaſt two or three ſeconds; and actually melted the glaſs on which they repoſed, (and as far as I can recollect,) without producing the ſmalleſt appearance of ſteam.

The water could not be decompoſed, for the iron was already ſaturated with oxygen.

This Experiment will, I fancy, be conſidered as affording an indiſputable proof that *intenſe Heat* may exiſt, at leaſt for a ſhort time, in a ſmall particle of matter ſurrounded by a cold Fluid.

Now, as it has been found by actual Experiment, that when a ſolution of nitro-muriate of gold in water is expoſed to the action of the ſun's rays, the gold is revived; and as it is known that an oxyd of gold may be reduced in the dry way, without addition, or merely by intenſe Heat, why ſhould we not conclude that it is merely by *Heat* that that metal is revived in the caſe under conſideration,—and that the *intenſity* of the Heat by which this oxygenation is effected, is preciſely the ſame in both caſes?

Should this ſuppoſition be admitted, we might, perhaps, venture to proceed one ſtep farther, and conſider the nature and progreſs of the mechanical operations which take place in diſoxygenation of metals, or their precipitation from a ſolution of their oxyds, when that operation is effected by means of Heat generated,—not by light,—but by the contact or union of infinitely ſmall particles of bodies, different in kind, and diſpoſed to generate or to abſorb ſenſible Heat on coming together; which particles being diſperſed about in the liquid ſolution, and in the ſubſtance added to it to effect the precipitation, are by this mixture brought into contact.

This would naturally lead us to an examination of the phenomena of ſolution,—and thoſe clearly underſtood would, no doubt, give us a diſtinct view of the mechanical operations by which thoſe tendencies to union are effected, which have been deſignated under the name *elective attraction*.

But how arduous an undertaking! what intenſe ſtudy!—what efforts of the imagination would be neceſſary to trace out and form diſtinct ideas of ſuch a ſucceſſion of events, all perfectly imperceptible by our organs, though aſſiſted by all the reſources of art!

Senſible of my own weakneſs, I dare not proceed any farther.—Perhaps it will be thought that I have already advanced much too far;—but it is right that I ſhould acknowledge fairly, that in the preſent caſe, the temerity I have ſhown has not been entirely without deſign.

There are two ways in which philoſophers, as well as other men, may be excited to action, and induced to engage zealouſly in the inveſtigation of any curious ſubject of inquiry :—they may be *enticed*,—and they may be *provoked*.

It will probaby not eſcape the penetration of my reader, that I have endeavoured to uſe both theſe methods.—I am well aware of the danger that attends the latter of them ; but the paſſionate fondneſs that I feel for the favourite objects of my purſuits frequently hurries me on far beyond the bounds which prudence would mark to circumſcribe my adventurous excurſions.

CHAP. IV.

An Account of a Variety of Miſcellaneous Experiments.—Thermometers with cylindrical Bulbs may be uſed to ſhow that Liquids are Non-conductors of Heat.—Ice-cold Water may be heated and made to boil ſtanding on Ice.—Remarkable Appearances attending the thawing of Ice, and the melting of Tallow, and of Bees-Wax, by means of the radiant Heat projected downwards by a red-hot Bullet.—Beautiful Cryſtals of Sea-Salt formed in Brine ſtanding on Mercury.—Olive Oil ſoon rendered colourleſs by Expoſure to the Air ſtanding on Brine.—An Attempt to cauſe radiant Heat from a red-hot Iron Bullet to deſcend in Oil.—Account of an artificial Atmoſphere in which horizontal Currents were produced by Heat.—Conjectures reſpecting the proximate Cauſes of the Winds.

THOUGH this Eſſay is already grown to a much larger ſize than I originally intended, and even larger than I could have wiſhed;—(well knowing how great an evil a great Book is generally thought to be)—I could not bring it to a concluſion without adding one Chapter more. In this Chapter the reader will find accounts of ſeveral Experiments, ſome of which he will probably conſider as not altogether unintereſting. To take up as little of his time as poſſible I ſhall be very brief in theſe accounts, and in general ſhall leave the reader to draw his own concluſions from the reſults of the Experiments I ſhall deſcribe.

§ 1. *An Account of ſeveral ſimple Experiments, which ſhow that Heat does not deſcend in Fluids.*

If a thermometer conſtructed with a long and narrow, naked cylindrical bulb,—(6 inches long, for inſtance, and ½ an inch in diameter,)—and filled with mercury, oil, ſpirits of wine, or any other Fluid proper for that purpoſe, with which it is required to make the Experiment in queſtion; ſuch thermometer being at the temperature of the air in ſummer, or at any temperature above the point of freezing water, if the lower end, or half, of its bulb be plunged into a glaſs tumbler filled quite full to the brim with pounded ice and water, the height of the Fluid in the tube of the inſtrument will ſhow that half the Fluid in the cylindrical bulb of the inſtrument is ice-cold, while the temperature of the other half of it remains unchanged.

The reſult will be the ſame, when, to prevent the communication of Heat from the air during the Experiment, that part of the bulb of the thermometer (the ſuperior half of it) which projects above the level of the top of the tumbler is covered with a ſheath lined with ſoft fur.

When more or leſs than half of the bulb of the thermometer is plunged into the ice and water, the height of the liquid in the tube of the inſtrument will ſhow, that that part only of the Fluid in the bulb is cooled which occupies the part of the bulb that is immerſed in the ice and water.

§ 2. *Ice-cold Water, ſtanding on Ice, may be heated and made to boil without melting the Ice, contrary to an Opinion that has generally prevailed.*

Take a thin glaſs tube, 1 inch in diameter, and about 8 or 10 inches long, containing about two or three inches of water, and by plunging the end of the tube into a freezing mixture of pounded ice and ſea-ſalt cauſe the water in the tube to congeal: this being done, pour two or three inches of ice-cold water on the ice; and wrapping up about two inches of the lower end of the tube with a piece of flannel, and holding it inclined at an angle of about 45°; by that part of it which is ſo covered, bring that part of the tube which is at the height of the ſurface of the Fluid-water to be juſt over the point of the flame of a burning candle, and diſtant from it about two or three inches. When the water in that part of the tube begins to boil, the tube may be advanced ſlowly over the flame of the candle; and if due care be taken to prevent a too ſudden application of the Heat, all the water in the tube to within one quarter of an inch of the ice may be brought into the moſt violent ebullition before the ice will begin to be melted, and at laſt will appear to boil even at the very ſurface of the ice.

§ 3. *The*

§ 3. *The radiant Heat from a red-hot Iron Bullet does not appear to be able to make its Way downwards through liquid Water, nor through melted Tallow, nor melted Wax.*

1ſt Experiment.—A very ſmall mercurial thermometer, with a naked globular bulb, was laid down in an horizontal poſition on two ſmall projections of wax, in the bottom of a ſhallow wooden diſh, in ſuch a manner that the engraved ſcale of the thermometer lying uppermoſt, the height of the mercury in its tube could be obſerved. This being done, I poured cold water into the diſh till it ſtood at the height of about ¼ of an inch above the bulb of the thermometer, and then preſented to the thermometer an iron bullet about 1½ inches in diameter, red-hot, which I held (by means of a fit handle) directly over its bulb at the diſtance of about an inch.

The thermometer ſeemed to take very little notice of the vicinity of the red-hot iron.

When its bulb was covered with oil the reſult of the Experiment was much the ſame; but when it was expoſed naked, or uncovered by a liquid, to the rays from the hot iron, it appeared to acquire Heat very rapidly. But the two following Experiments were ſtill more deciſive and ſatisfactory.

2d Experiment.—A ſhallow earthen diſh, about 3 inches deep and 12 inches in diameter at its brim, was filled with water, and being expoſed in a cold room in winter, the water was frozen, and formed a cake a cake of ice at its ſurface, about an inch thick. Letting the diſh remain in its place, in order that the ſurface of the ice might remain perfectly horizontal, (which was neceſſary to the complete ſucceſs of the Experiment, as will preſently be ſeen) I entered the room with a chafing-diſh filled with live coals, in the midſt of which was my iron bullet, perfectly red-hot; and taking out the bullet from among thoſe burning coals, I held it over the centre of this horizontal ſheet of ice, and diſtant from it about $\frac{1}{10}$ of an inch.

The ice directly under the red-hot bullet was ſoon thawed; but the depth to which it was thawed was very inconſiderable: the water, however, extended itſelf ſlowly from the centre towards the circumference, and at length a circular ſpot 2 or 3 inches in diameter in the centre of the ſurface of the ice was covered with it, though but to a very inconſiderable depth.

This little ſpreading ſea appeared to prey on the wall of ice by which it was ſurrounded on every ſide.

The particles of water in contact with this wall, being rendered ſpecifically lighter on becoming ice-cold, they move upwards, and making way for other warmer particles to advance from below, cauſe currents in oppoſite directions to ſet between the centre (where the hot-iron remains) and the circumference.—As a current at the temperature of 41° muſt neceſſarily ſet downwards at the middle of the circle, this current ſtriking againſt the middle of the excavation formed in the ice ought to deepen it gradually in that part, though but ſlowly,—and this is what was actually found to be the caſe; for the bottom of this excavation was not perfectly flat, but was deeper at and near its centre than at its ſides.

3d Experiment.—When this Experiment was varied by uſing a flat cake of tallow inſtead of a cake of ice, a very extraordinary appearance indeed preſented itſelf, which at firſt ſurpriſed me very much, but which I ſoon perceived was a new, and very ſtriking proof that Fluids are non-conductors of Heat.

The bottom of the circular cavity in the cake of tallow which was occupied by that part of the tallow that had been melted in the Experiment, inſtead of being concave, as I had found that in the ice to be,—or flat,—as I expected to find this,—was *convex* in the middle, or rather roſe up in the form of a protuberance, or very blunt point, the extremity of which reached almoſt to the ſurface of the melted tallow! As the iron bullet was held as near as poſſible to the tallow, the end of this projection, which remained unmelted, was certainly not more than $\frac{2}{10}$ of an inch diſtant from this red-hot ball! Reflecting on the unexpected reſult of this Experiment I was much ſtruck, and not a little humiliated, with the proof it ſeemed to me to afford of the impoſſibility of predicting with certainty any event, however inevitable it may appear, which has not actually been ſeen to happen.

Though I well knew how the Heat muſt be communicated under the given circumſtances, and could foretell with certainty the directions of the currents it muſt neceſſarily occaſion in the melting tallow; yet, the utmoſt efforts of my intellectual powers, exerciſed as they were by much meditation, were not ſufficient to enable me to foreſee that the point where leaſt Heat would be communicated was that preciſely which was neareſt to the red-hot bullet; and that a protuberance of unmelted tallow would be left in that place.

Let thoſe be very cautious who ſpeculate on the ſuppoſed reſults of Experiments they have never made!

On repeating this Experiment, and varying it by uſing a cake of fine bleached *bees-wax*, inſtead of tallow, the reſult was much the ſame: the protuberance, however, in the middle of the circular cavity occupied by the melted wax, though perfectly perceptible, was leſs conſiderable, in height, than that in the cake of tallow.

§ 4. *Beautiful Cryſtals of Sea-Salt formed in Brine ſtanding on Mercury.*

A ſmall quantity of ſtrong brine, ſtanding on mercury in an open glaſs tumbler, having by accident been left in a room in a retired part of the houſe, I obſerved at the end of about ſix months, that two beautiful cryſtals of ſalt, perfectly quadrangular, had been formed in it, one of which was $\frac{14}{40}$ of an inch long, $\frac{11}{40}$ of an inch wide, and $\frac{5}{40}$ of an inch in thickneſs; and the other $\frac{14}{40}$ of an inch long, $\frac{10}{40}$ of an inch wide, and $\frac{11}{80}$ of an inch thick.

Did the Fluid mercury on which this brine repoſed contribute?—and how?—to the regularity of the form, and the uncommon ſize of theſe cryſtals?—And might not beautiful cryſtals of other ſalts be procured by ſimilar means?

§ 5. *Olive Oil rendered colourleſs by Expoſure to the Air ſtanding on Brine.*

A quantity of *olive oil*, about $\frac{1}{4}$ of an inch in depth, having by accident been left ſtanding in an open glaſs jar, about four inches in diameter, on about a quart of brine, moderately ſtrong, in a retired room, where the ſun's rays never enter; at the end of about ſix months I obſerved that the oil had become perfectly colourleſs, and appeared to me to be nearly as tranſparent as the pureſt water. On the approach of winter I found that this oil was much more liable to be congealed with cold than oil of the ſame kind which had ſtood near it many months in a large glaſs bottle cloſed with a cork.

§ 6. *An unſucceſsful Attempt to cauſe radiant Heat from a red-hot Iron Bullet to deſcend in Oil.*

Having poured a quantity of this colourleſs oil into a glaſs tumbler, and cauſed it to congeal throughout, I preſented to its upper ſurface a red-hot iron bullet, $1\frac{1}{2}$ inches in diameter, and held it quite cloſe to the oil, ſeveral minutes, till the bullet ceaſed to be red-hot. As the oil ſeemed rather to be merely thickened by the cold, and to have loſt its tranſparency in conſequence of the preſence of a number of opaque particles, which were everywhere diſperſed about in it, than to be congealed into a ſolid maſs, I thought that if it were poſſible for radiant Heat to deſcend in any Fluid it might perhaps be in this; and if this ſhould happen, I was certain to make the diſcovery by the manner in which the oil recovered its tranſparency; for ſhould radiant Heat deſcend, the form of the maſs of oil firſt reſtored to its tranſparency muſt neceſſarily have been *hemiſpherical*, or ſome ſection of a ſphere, or at leaſt of ſome convex figure: but the under part of that part of the oil which was reſtored to its tranſparency in this Experiment was, to all appearance, as perfectly flat and horizontal as the upper ſurface of it, which proves that the Heat, by which the congealed oil was thawed, was communicated to it,—not immediately by the red-hot bullet,—but *mediately* by means of the Heat abſorbed by or generated in the ſides of the tumbler. This Experiment appears to me to be important in many reſpects; but it would be foreign to my preſent purpoſe to engage in an inveſtigation of the ſubject with which it is moſt intimately connected.

I cannot finiſh this Eſſay without giving my reader an account of one more Experiment, the reſult of which was not only quite unexpected, but uncommonly intereſting.

Happening accidentally to place in a window the little inſtrument I had contrived for rendering viſible the internal motions which are occaſioned in water when Heat is propagated in that Fluid *, as it was winter, and the room was warmed by a German ſtove, that ſide of the inſtrument which happened to be neareſt the window being expoſed to a current of cold air, while the inſtrument received Heat continually on the other ſide from the warmer air of the room, the liquid in the inſtrument was thrown into motions which never ceaſed, and afforded a very intereſting ſight.

With a view merely to amuſe myſelf, and the friends who ſhould happen to call in to viſit me,—and without the ſmalleſt expectation of making any new diſcoveries,—I contrived, and cauſed to be executed, the inſtrument I am now about to deſcribe, which I thought could not fail to render theſe motions perpetual, and exhibit them in a ſtriking manner.

A flat box was formed of two equal panes, each 13 inches high, and 10½ inches wide, of fine ground glaſs, fitted into a ſquare frame of braſs in ſuch a manner, that theſe two panes (which are parallel to each other) are at the diſtance of 1 inch from each other. In the middle of the top of this braſs frame there is a circular opening about ½ an inch in diameter, into which a projecting cylindrical braſs tube, about half an inch in length, is ſoldered; and in the middle of the bot-

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* For a deſcription of this inſtrument ſee Chapter II. of this Eſſay.

tom of the frame there is a ſimilar tube which projects downward. The firſt of theſe openings ſerves for introducing into the flat box the liquid with which it is filled; and the other for drawing it off; and they are both well cloſed with fit ſtopples of cork.

On both ſides of this braſs frame there are deep grooves into which the panes of glaſs are fitted, and the box was made water-tight by luting the joinings of the glaſs with the frame with glaziers putty. On the outſide of the frame there are thin projections of ſheet braſs, by means of which the box was fixed in one of the ſaſhes of a window in my room, where it occupied the place of a pane of glaſs, which was removed to make way for it. This window fronts the ſouth-eaſt, and conſequently is expoſed to the ſun a great part of the day.

Having provided a ſufficient quantity of the ſaline ſolution,—(of the ſame kind as was uſed in conſtructing the inſtrument above mentioned, contrived for rendering viſible the internal motions in Fluids) and having mixed with it a due proportion of pulverized yellow amber, I now filled the box half full with this mixture; and as the air in the room was conſiderably warmer than that without, I expected that the motions in the liquid occaſioned by the paſſage of the Heat would immediately commence.

This actually happened;—but how great was my ſurpriſe, when, inſtead of the vertical currents I expected, I diſcovered horizontal currents, running in oppoſite directions,—one above another,—or regular WINDS,—which, ſpringing up in the different regions of this artificial atmoſphere, prevailed for a long time with the utmoſt regularity;—while the ſmall particles of the amber collecting themſelves together formed clouds of the moſt fantaſtic forms, which being carried by the winds, rendered the ſcene perfectly faſcinating!

It would be impoſſible to deſcribe the avidity with which I gazed on theſe [illegible]ting appearances.

In the ſtate of enthuſiaſm I then was, it really ſeemed to me that Nature had for a moment drawn back the veil with which ſhe hides from mortal eyes her moſt ſecret and moſt intereſting operations;—and that I now ſaw the machinery at work by which winds and ſtorms are raiſed in the atmoſphere!

Nothing ſeemed to be wanting to complete this bewitching ſcene, and give it the air of perfect enchantment, but that lightning, in miniature, ſhould burſt from theſe little clouds: and they were frequently ſo thickened up, and had ſo much the appearance of preparing for a ſtorm, that had that event actually taken place, it could hardly have increaſed my wonder and ecſtaſy.

There were ſeveral accidental circumſtances attending this Experiment, which contributed to render it more intereſting. The ſun, which happened to be remarkably bright, ſhone full upon the window where the apparatus was placed; and as the grooves in the frame in which the plates of glaſs were fixed were not deep, that part of this frame which formed the narrow bottom of the box being expoſed to the ſun's rays, a conſiderable quantity of Heat was generated by them in that place, as appeared by the motions of the particles of pulverized amber which lay on the bottom of the box, or thoſe which were brought there by the currents.

When theſe particles, on being heated by the ſun-beams, began to move, they firſt aroſe up nearly perpendicularly; but before they had riſen to any conſiderable height, they were carried away obliquely, and nearly in an horizontal direction, by the lower current, anſwering to the wind which, in the atmoſphere, prevails at the ſurface of the earth.

The perpendicular riſe of theſe particles from the bottom of the box, and the ſubſequent change of their direction, called to my remembrance an ~~appearance very~~ common in hot countries, which I recollected to have often ſeen, and by which I had often been amuſed in my youth: in very hot and dry weather, when the wind is ſtill and the ſun very powerful, the air which lies on the ground often appears in the moſt violent agitation, reſembling that of a boiling liquid; which motion is moſt rapid at the ſurface of the earth, and appears to ceaſe at the height of five or ſix feet above the ground.

Is not this violent agitation occaſioned by the conflict which takes place between the hot and the comparatively cold air moving *vertically*, and in oppoſite directions, very near the ſurface of the ground? And are not the winds which prevail above, occaſioned by the efforts of whole *ſtrata* of air to aſcend or deſcend obliquely?

The currents I obſerved to prevail in my artificial atmoſphere were never perfectly horizontal; and if my ſuſpicions with reſpect to the cauſe of the winds are well founded, neither can thoſe winds be horizontal which prevail in the ſuperior regions of the atmoſphere of the earth, though they may be very nearly ſo.

The greateſt velocity of the currents in the ſaline liquid in this Experiment was nearly two inches in a minute, but their motions were in general much ſlower. As the windows in the room in which this Experiment was made are double, (as are all thoſe both in ſummer and winter in the apartment I inhabit) and as the apparatus above deſcribed occupied the place of a pane of glaſs belonging to the inſide-window, it was in my power, by opening either the inſide-window or the outſide-window, to cauſe the Heat on the two oppoſite ſides of the box to be either equal or unequal at pleaſure; and by variations which that arrangement enabled me to make in the Experiments I produced ſeveral intereſting appearances.

There was one very ſtriking appearance indeed, which never failed to preſent itſelf regularly every day during the three weeks that the Experiment was continued *. The clouds, after having been driven about all day by the different currents in the liquid, (of which there were ſometimes as many as ſix or ſeven, running in oppoſite directions at the ſame time) never failed to collect themſelves together in the evening, into large maſſes; ſometimes forming only one, and ſometimes two or three *ſtrata* at different heights, where they remained, to all appearance perfectly motionleſs, during the night.

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* An end was put to the Experiment by an accident; the box being broken by the careleſſneſs of a ſervant in ſhutting the window-ſhutter.

There can be no queſtion with reſpect to the *proximate* cauſe of this phænomenon; for it was undoubtedly owing to a diminution or total ceſſation of the operation of that cauſe,—of thoſe cauſes,—or of ſome of them,—by which an inequality of temperature in the liquid was produced and continued;—but it would be highly curious to inveſtigate the more remote cauſes of this appearance, and ſee how far *light*, or rather the abſence of it, was concerned in producing it: but that diſcuſſion would lead me into a very abſtruſe inquiry,—that reſpecting radiant Heat,—which would take up more time than I am at preſent able to beſtow on it. Perhaps I may find leiſure and courage at ſome future period to attempt that moſt difficult inveſtigation. My reader will doubtleſs have obſerved that I have hitherto taken pains to avoid it.

I cannot take my leave of the Experiment I have been deſcribing without giving my reader a faithful account of every thing I can recollect reſpecting it; and particularly of one accidental circumſtance, which, it is poſſible, may have had ſome ſhare in producing the intereſting appearances which ſo powerfully attracted my attention.

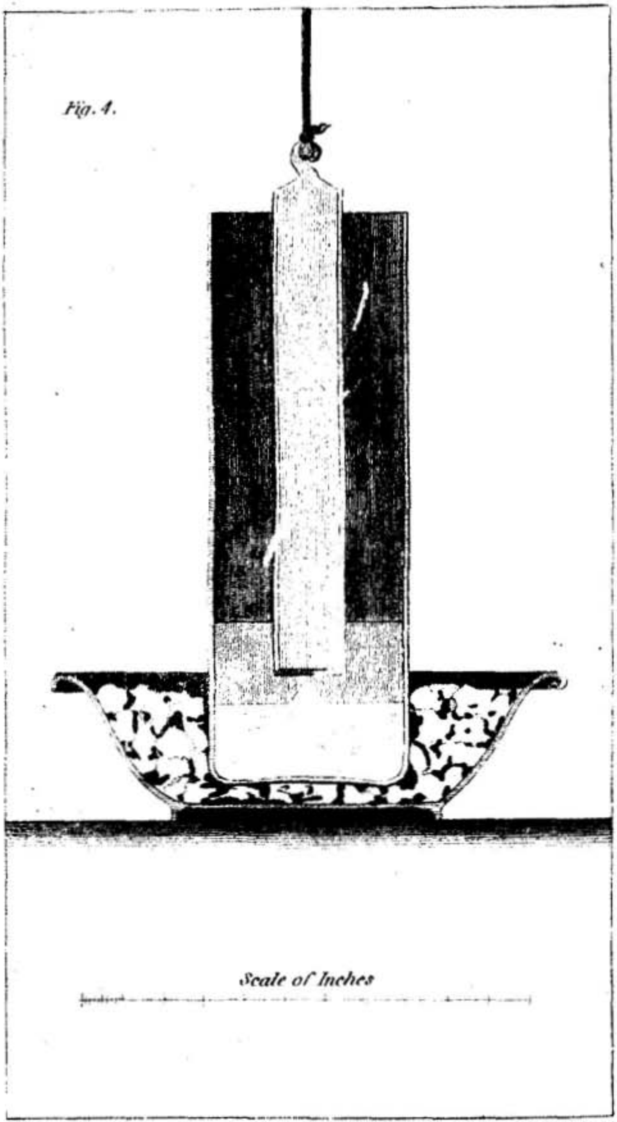
The ſaline liquor and the pulverized amber were mixed in a bottle, and were not put into the flat box till after it had been fixed in the ſaſh or frame of the window; but when I came to pour this mixture into the box I found that I had not provided enough of it. To ſupply this defect, without the trouble of emptying the box, I added, at ſeveral different times, pure water, and a ſtrong ſolution of pot-aſh, in ſuch proportions as I knew to be proper to produce the ſpecific gravity required; and then endeavoured to mix the whole as intimately as poſſible by agitating the liquor for ſome conſiderable time by means of a long and ſtrong quill, the end of which I thruſt down into the box through the hole by which the liquor was introduced.

Whether thoſe different portions of liquid were in fact intimately mingled by theſe means, I cannot poſitively determine. They certainly had every appearance of being ſo; for the amber was evidently well mixed, and very equally diſtributed in every part of the Fluid. But even ſhould we grant that the liquid remained divided in different *ſtrata*, arranged according to the ſpecific gravities of the different portions of it that were poured into the box at different times, it does not appear to me that the reſult of the Experiment would be leſs intereſting on that account, or the application of it leſs ſatisfactory in explaining the cauſe of the winds in the atmoſphere.

I am, however, far from being deſirous that much ſtreſs ſhould be laid on this ſingle Experiment, being perfectly ſenſible that others may be contrived, the reſults of which would be more deciſive: in the mean time it appears to me that the hint given us is too plain not to deſerve ſome attention. If it ſhould awaken the curioſity of experimental philoſophers, and excite them to farther inveſtigation, the end I had principally in view in publiſhing this account of it will be completely anſwered.

Plate 3.

Fig. 4.

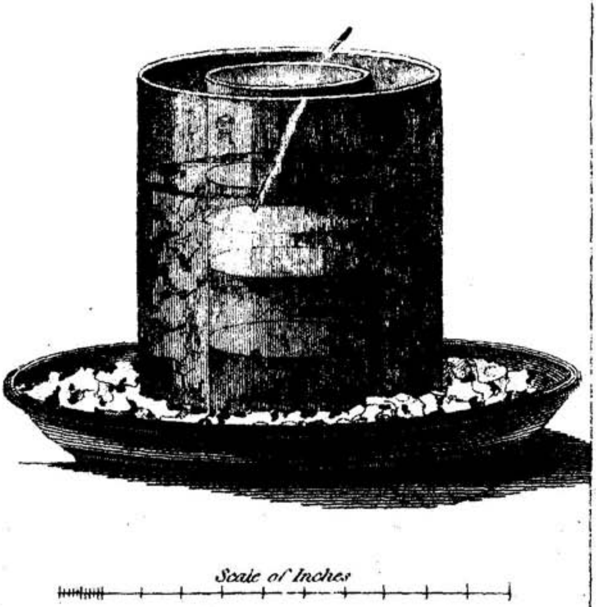


Neele sculp Strand

London Pub.d May 1st 1798 by Cadell & Davies Strand.

Plate 4.

Fig. 5



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DESCRIPTION OF THE PLATES.

PLATE III.

FIG. 4. This Figure repreſents a vertical ſection of the apparatus uſed in the Experiment No. 55. (ſee page 315), in which an attempt was made to melt the top of a projecting point of ice by Heat tranſmitted *downwards* through olive-oil communicated by a ſolid cylinder of iron, heated in boiling water.

In this Figure the tall glaſs jar (in the bottom of which the cake of ice was frozen) is ſtanding in an earthen pan filled with pounded ice.

The *oil* is alſo repreſented ſtanding on the *cake of ice* in the jar; and the iron cylinder in its ſheath of paper ſuſpended in the axis of the jar in ſuch a manner that the lower end of this cylinder, which is flat, is directly over the pointed projection of ice, and diſtant from it $\frac{2}{10}$ of an inch.

PLATE IV.

Fig. 5. This Figure ſhows the manner in which the Experiment No. 57. (ſee page 326) was made, when pure or freſh water in a glaſs jar was made

to repoſe on brine, or water ſaturated with ſea-ſalt, without mixing with it.

In this Experiment the ſmaller jar, which contained the brine, the pure water, and a quantity of olive-oil by which the ſurface of the pure water was covered, ſtood in a larger glaſs jar,—which laſt ſtood in a ſhallow earthen diſh filled with pounded ice and water.

The ſpace between the outſide of the ſmaller jar and the inſide of the larger jar was filled, to the height of about an inch above the level of the ſurface of the oil in the ſmaller jar, with pieces of ice nearly as large as walnuts, and ice-cold water.

END OF PART II. OF ESSAY VII.

CONTENTS

OF THE

EIGHTH ESSAY.

CHAP. I.

AN Account of the Inſtruments that were prepared for making the propoſed Experiments.—A Thermometer is conſtructed whoſe Bulb is ſurrounded by a TORRICELLIAN VACUUM.—*Heat is found to paſs in a Torricellian Vacuum with greater Difficulty than in Air.—Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat determined by Experiment.—Relative conducting Powers of dry Air and of moiſt Air.—Relative conducting Powers of Air of different Degrees of Denſity.—Relative conducting Powers of* MERCURY; WATER; AIR; *and a* TORRICELLIAN VACUUM. Page 391

CHAP. II.

The relative Warmth of various Subſtances uſed in making artificial Cloathing, determined by Experiment.—Relative Warmth of Coverings of the ſame Thickneſs, and formed of the ſame Subſtance, but of different Denſities.—Relative Warmth of

Coverings formed of equal Quantities of the ſame Subſtance, diſpoſed in different Ways.—Experiments made with a View to determining how far the Power which certain Bodies poſſeſs of confining Heat depends on their chemical Properties.—Experiments with Charcoal*—with* Lampblack*—with* Woodaſhes*—Striking Experiments with* Semen Lycopodii.*—All theſe Experiments indicate that the Air which occupies the Interſtices of Subſtances uſed in forming Coverings for confining Heat, acts a very important Part in that Operation.—Thoſe Subſtances appear to prevent the Air from conducting the Heat.—An Inquiry concerning the Manner in which this is effected.—This Inquiry leads to a deciſive Experiment from the Reſult of which it appears that* Air is a perfect Non-conductor of Heat.*—This Diſcovery affords the Means of explaining a Variety of intereſting Phenomena in the Œconomy of Nature.* Page 428

ESSAY IX.

An INQUIRY concerning the SOURCE of the HEAT which is EXCITED by FRICTION. Page 467

ESSAY VIII.

OF THE PROPAGATION OF HEAT IN VARIOUS SUBSTANCES:

BEING

An Account of a Number of New Experiments made with a View to the Inveſtigation of the Causes of the Warmth of Natural and Artificial Clothing.

First publiſhed in the Philoſophical Tranſactions.

INTRODUCTION.

THIS Eſſay contains nothing that will be new to philoſophical readers; for it is little more than the ſubſtance of two Papers which have already appeared in the Philoſophical Tranſactions of the Royal Society of London; one in the year 1786; and the other (for which the Author had the honour to receive from the Society the Copleian Annual Medal) in the year 1792.

As reference has frequently been made to theſe Papers in ſeveral of the preceding Eſſays; and as many of the Experiments of which an account is given in them are not only intereſting in themſelves, but are neceſſary to be known in all their details in order to judge of ſeveral important concluſions that have been founded on their reſults, the Author has thought that it would not be improper to republiſh them under the preſent form. He was alſo deſirous of adding the ſubſtance of thoſe Papers to his Sixth and Seventh Eſſays, in order that all that he has written on the *Science of Heat* might be brought together in one volume.

The Eſſays which are deſtined to compoſe the next volume (many of which are already in great forwardneſs) are all on practical ſubjects of a popular nature, and of general utility; and on that account it was judged beſt to keep them ſeparate from thoſe contained in this volume, which partake more of the nature of abſtruſe philoſophical inveſtigations.

Various unforeſeen events have contributed to retard the publication of the promiſed Eſſays on Kitchen Fire-places—on Cottage Fire-places—and on Clothing; but the Author has well-founded hopes of being able to bring them forward in the courſe of a few months.

Fig. 1.

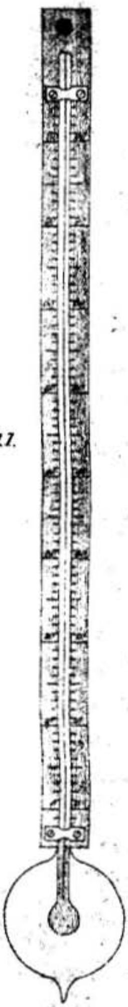


Fig. 2.

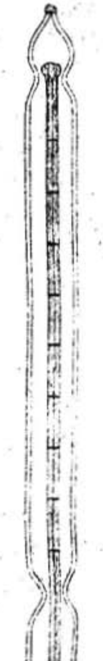


Fig. 3.

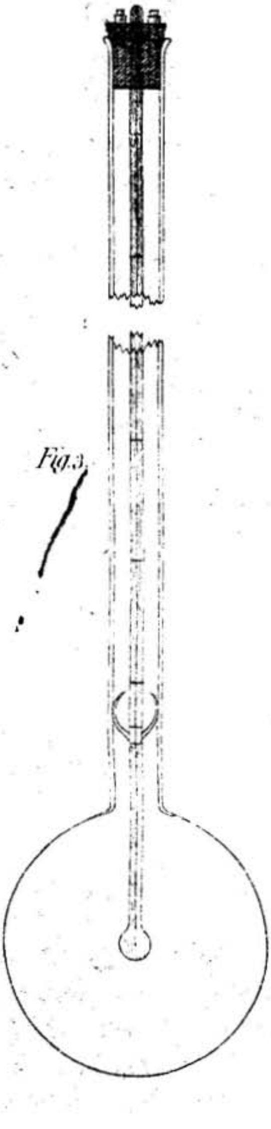
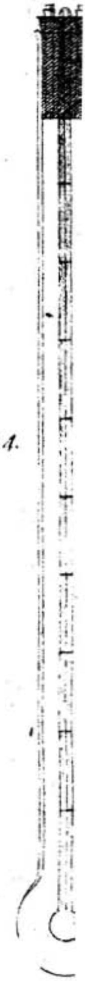


Fig. 4.



Scale of Inches

ESSAY VIII.

Of the PROPAGATION of HEAT in various Subſtances.

CHAP. I.

An Account of the Inſtruments that were prepared for making the propoſed Experiments.—A Thermometer is conſtructed whoſe Bulb is ſurrounded by a TORRICELLIAN VACUUM.—*Heat is found to paſs in a Torricellian Vacuum with greater Difficulty than in Air.—Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat determined by Experiment.—Relative conducting Powers of dry Air and of moiſt Air.—Relative conducting Powers of Air of different Degrees of Denſity.—Relative conducting Powers of* MERCURY; WATER; AIR; *and a* TORRICELLIAN VACUUM.

[Read before the ROYAL SOCIETY, March 9, 1786.]

EXAMINING the conducting power of air, and of various other fluid and ſolid bodies, with regard to Heat, I was led to examine the conducting power of the *Torricellian vacuum*. From the ſtriking analogy between the electric fluid and Heat reſpecting their conductors and non-conductors, (having found that bodies, in general, which are conductors of the electric fluid, are likewiſe good conductors of Heat, and, on the contrary, that electric bodies, or ſuch as are bad conductors of the electric fluid, are likewiſe bad conductors of Heat,) I was led to imagine that the Torricellian vacuum, which is known to afford ſo ready a paſſage to the electric fluid, would alſo have afforded a ready paſſage to Heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I conceived to be inadequate to determining this queſtion; not only on account of the impoſſibility of making a perfect void of air by means of the pump; but alſo on account of the moiſt vapour, which exhaling from the wet leather and the oil uſed in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of Heat: I had recourſe therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whoſe bulb (which was globular) was juſt half an inch, Paris meaſure, and fixed it in the centre of a hollow glaſs ball of the diameter of $1\frac{3}{4}$ Paris inch, in ſuch a manner, that the ſhort neck or opening of the ball being ſoldered faſt to the tube of the thermometer $7\frac{1}{2}$ lines above its bulb, the bulb of the thermometer remained fixed in the centre of the ball, and conſequently was cut off from all communication with the external air. In the bottom of the glaſs ball was fixed a ſmall hollow tube or point, which projecting outwards was ſoldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the ſpace between the internal ſurface of the glaſs ball and the bulb of the thermometer was filled with hot mercury, which had been previouſly freed of air and moiſture by boiling. The ball, and alſo the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The inſtrument now became a barometer, and the mercury deſcending from the ball (which was now uppermoſt) left the ſpace ſurrounding the bulb of the thermometer free of air. The mercury having totally quitted the glaſs ball, and having ſunk in the tube to the height of 28 inches, (being the height of the mercury in the common barometer at that time) with a lamp and a blow-pipe I melted the tube together, or ſealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I ſeparated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now poſſeſſed a thermometer whoſe bulb was confined in the centre of a *Torricellian vacuum*, and which ſerved at the ſame time as the body to be heated, and as the inſtrument for meaſuring the Heat communicated.

Experiment, N° 1.

With this inſtrument (ſee Fig. 1.) I made the following Experiment. Having plunged it into a veſſel filled with water, warm to the 18th degree of REAUMUR's ſcale, and ſuffered it to remain there till it had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometer ſtood at 18°, I took it out of this veſſel and plunged it ſuddenly into a veſſel of boiling water, and holding it in the water (which was kept conſtantly boiling) by the end of the tube, in ſuch a manner that the glaſs ball, in the centre of which was the bulb of the thermometer, was juſt ſubmerged, I obſerved the number of degrees to which the mercury in the thermometer had ariſen at different periods of time, counted from the moment of its immerſion. Thus, after it had remained in the boiling water 1 min. 30 ſec. I found the mercury had riſen from 18° to 27°. After 4 minutes had elapſed, it had riſen to 44°$\frac{9}{10}$; and at the end of 5 minutes it had riſen to 48°$\frac{2}{10}$.

Experiment, N° 2.

Taking it now out of the boiling water I ſuffered it to cool gradually in the air, and after it had acquired the temperature of the atmoſphere, which was that of 15° R. (the weather being perfectly fine) I broke off a little piece from the point of the ſmall tube which remained at the bottom of the glaſs ball, where it had been hermetically ſealed, and of courſe the atmoſpheric air ruſhed immediately into the ball. The ball ſurrounding the bulb of the thermometer being now filled with air, (inſtead of being emptied of air, as it was in the before-mentioned Experiment,) I reſealed the end of the ſmall tube at the bottom of the glaſs ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the inſtrument ſo prepared I repeated the Experiment before-mentioned; that is to ſay, I put it into water warmed to 18°, and when it had acquired the temperature of the water, I plunged it into boiling water, and obſerved the times of the aſcent of the mercury in the thermometer. They were as follows:

	Time elapſed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, - - - -		18° R.
	M. S.	°
After having remained in the boiling water	0 45	27
	1 0	$34\frac{4}{10}$
	2 10	$44\frac{9}{10}$
	2 40	$48\frac{2}{10}$
	4 0	$56\frac{2}{10}$
	5 0	$60\frac{\text{[illegible]}}{10}$

From the ruſult of theſe Experiments it appears evidently, that the Torricellian vacuum, which affords ſo ready a paſſage to the electric fluid, ſo far from being a good conductor of Heat, is a much worſe conductor of it than common air, which of itſelf is reckoned among the worſt: for in the laſt Experiment, when the bulb of the thermometer was ſurrounded with air, and the inſtrument was plunged into boiling water, the mercury roſe from 18° to 27° in 45 ſeconds; but in the former Experiment, when it was ſurrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 ſeconds = 90 ſeconds, to acquire that degree of heat. In the vacuum it required 5 minutes to riſe to 48°$\frac{2}{10}$; but in air it roſe to that height in 2 minutes 40 ſeconds; and the proportion of the times in the other obſervations is nearly the ſame, as will appear by the following Table.

	The bulb of the thermometer placed in the centre of the glaſs ball, and			
	ſurrounded by a Torricellian vacuum.		ſurrounded by air.	
	(Exp. N° 1.)		(Exp. N° 2.)	
	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
Upon being plunged into boiling water - -		18°		18°
	M. S.	°	M. S.	°
After remaining in it	1 30	27	0 45	27
	——	——	1 0	$30\frac{4}{10}$
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 0	$48\frac{2}{10}$	2 40	$48\frac{2}{10}$
	——	——	4 0	$56\frac{2}{10}$
	——	——	5 0	$60\frac{9}{10}$

Theſe Experiments were made at Manheim, upon the firſt day of July 1785, in the preſence of Profeſſor Hemmer, of the Electoral Academy of Sciences of Manheim, and Charles Artaria, meteorological inſtrument maker to the academy, by whom I was aſſiſted in making them.

Finding the conſtruction of the inſtrument made uſe of in theſe Experiments attended with much trouble and riſk, on account of the difficulty of ſoldering the glaſs ball to the tube of the thermometer without at the ſame time either cloſing up, or otherwiſe injuring, the bore of the tube, I had recourſe to another contrivance much more commodious, and much eaſier in the execution.

At the end of a glaſs tube or cylinder about eleven inches in length, and near three quarters of an inch in diameter internally, I cauſed a hollow globe to be blown 1$\frac{1}{2}$ inch in diameter, with an opening in the bottom of it correſponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or ſhort tube, about an inch in length. Having a thermometer prepared, whoſe bulb was juſt half an inch in diameter, and whoſe freezing point fell at about 2$\frac{3}{4}$ inches above its bulb, I graduated its tube according to Reaumur's ſcale, beginning at 0°, and marking that point, and alſo every tenth degree above it to 80°, with threads of fine ſilk bound round it, which being moiſtened with lac varniſh adhered firmly to the tube. This thermometer I introduced into the glaſs cylinder and globe juſt deſcribed, by the opening in the bottom of the globe, having firſt choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its ſides inwards towards its axis, leaving only an opening ſufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in ſuch a manner that the centre of its bulb coincided with the centre of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the incloſed thermometer, and taking out the thermometer, I choaked the cylinder again in this place.

Introducing now the thermometer for the laſt time, I cloſed the opening at the bottom of the globe at the lamp, taking care before I brought it to the fire, to turn the cylinder upſide down, and to let the bulb of the thermometer fall into the cylinder till it reſted upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now cloſed, and the bulb of the thermometer being ſuffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as poſſible in the centre of the globe, and obſerving and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upſide down, and ſuffering the bulb of the thermometer to enter the cylinder, and reſt upon the firſt or lower choak, (by which means the end of the tube of the thermometer came further out of the cylinder) the end of the tube was cut off at the mark juſt mentioned, (care having firſt been taken to melt the internal cavity or bore of the tube together at that place) and a ſmall ſolid ball of glaſs, a little larger than the internal diameter or opening of the choak, was ſoldered to the end of the tube, forming a little button or knob, which reſting upon the upper choak of the cylinder ſerved to ſuſpend the thermometer

meter in ſuch a manner that the centre of its bulb coincided with the centre of the globe in which it was ſhut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the fruſtum of a hollow cone, the end of it was ſoldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically ſealed, the barometrical tube was detached from it with a file, and the thermometer was left completely ſhut up in a Torricellian vacuum, the centre of the bulb of the thermometer being confined in the centre of the glaſs globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube. (See Fig. 2.)

Of theſe inſtruments I provided myſelf with two, as nearly as poſſible of the ſame dimenſions; the one, which I ſhall call N° 1. being voided of air, in the manner above deſcribed; the other, N° 2. being filled with air, and hermetically ſealed.

With theſe two inſtruments (ſee Fig. 2.) I made the following Experiments upon the 11th of July laſt at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the academy of Manheim at 47°

Experiments, N° 3, 4, 5, and 6.

Putting both the inſtruments into a mixture of pounded ice and water, I let them remain there till the mercury in the incloſed thermometers reſted at the point 0°, that is to ſay, till they had acquired exactly the temperature of the cold mixture; and then taking them out of it I plunged them ſuddenly into a large veſſel of boiling water, and obſerved the time required for the mercury to riſe in the thermometers from ten degrees to ten degrees, from 0° to 80°, taking care to keep the water conſtantly boiling during the whole of this time, and taking care alſo to keep the inſtruments immerſed to the ſame depth, that is to ſay, juſt ſo deep that the point 0° of the incloſed thermometer was even with the ſurface of the water.

Theſe Experiments I repeated twice with the utmoſt care; and the following Table gives the reſult of them.

<table>
<tr><td colspan="3"><i>Thermometer</i> N° 1.
Its bulb half an inch in diameter, ſhut up in the centre of a hollow glaſs globe, 1½ inch in diameter, <i>void of air</i>, and hermetically ſealed.
<i>Taken out of freezing water, and plunged into boiling water.</i></td><td colspan="3"><i>Thermometer</i> N° 2.
Its bulb half an inch in diameter, ſhut up in the centre of a hollow glaſs globe, 1½ inch in diameter, <i>filled with air</i>, and hermetically ſealed.
<i>Taken out of freezing water, and plunged into boiling water.</i></td></tr>
<tr><td colspan="2">Time elapſed.</td><td rowspan="2">Heat acquired.</td><td colspan="2">Time elapſed.</td><td rowspan="2">Heat acquired.</td></tr>
<tr><td>Exp. N° 3.</td><td>Exp. N° 4.</td><td>Exp. N° 5.</td><td>Exp. N° 6.</td></tr>
<tr><td></td><td></td><td>0°</td><td></td><td></td><td>0°</td></tr>
<tr><td>M. S.</td><td>M. S.</td><td>°</td><td>M. S.</td><td>M. S.</td><td>°</td></tr>
<tr><td>0 51</td><td>0 51</td><td>10</td><td>0 30</td><td>0 30</td><td>10</td></tr>
<tr><td>0 59</td><td>0 59</td><td>20</td><td>0 35</td><td>0 37</td><td>20</td></tr>
<tr><td>1 1</td><td>1 2</td><td>30</td><td>0 41</td><td>0 41</td><td>30</td></tr>
<tr><td>1 18</td><td>1 22</td><td>40</td><td>0 49</td><td>0 53</td><td>40</td></tr>
<tr><td>1 24</td><td>1 23</td><td>50</td><td>1 1</td><td>0 59</td><td>50</td></tr>
<tr><td>2 0</td><td>1 51</td><td>60</td><td>1 24</td><td>1 20</td><td>60</td></tr>
<tr><td>3 30</td><td>3 6</td><td>70</td><td>2 45</td><td>2 25</td><td>70</td></tr>
<tr><td>11 41</td><td>10 27</td><td>80</td><td>9 10</td><td>9 38</td><td>80</td></tr>
<tr><td>22 44</td><td>21 1</td><td>= total time</td><td>16 55</td><td>17 3</td><td>= total time</td></tr>
<tr><td colspan="3">of heating from 0° to 80°.
Total time from 0° to 70°:
M. S.
In Exp. N° 3. = 11 3
In Exp. N° 4. = 10 34
Medium = 10 48½</td><td colspan="3">of heating from 0° to 80°.
Total time from 0° to 70°:
M. S.
In Exp. N° 5. = 7 45
In Exp. N° 6. = 7 25
Medium = 7 35</td></tr>
</table>

It appears from theſe Experiments that the conducting power of air to that of the Torricellian vacuum, under the circumſtances deſcribed, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{2}}{60}$ inverſely, or as 1000 to 702 nearly; for the quantities of Heat communicated being equal, the intenſity of the communication is as the times inverſely.

In theſe Experiments the Heat paſſed through the ſurrounding medium *into* the bulb of the thermometer:

meter: in order to reverſe the Experiment, and make the Heat paſs *out of* the thermometer, I put the inſtruments into boiling water, and let them remain therein till they had acquired the temperature of the water; that is to ſay, till the mercury in the incloſed thermometers ſtood at 80°; and then, taking them out of the boiling water, I plunged them ſuddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I obſerved the times employed in cooling as follows:

Thermometer N° 1. Surrounded by a *Torricellian vacuum.* *Taken out of boiling water, and plunged into freezing water.*			*Thermometer* N° 2. Surrounded by *air.* *Taken out of boiling water, and plunged into freezing water.*		
Time elapſed.		Heat loſt.	Time elapſed.		Heat loſt
Exp. N° 7.	Exp. N° 8.		Exp. N° 9.	Exp. N° 10.	
———	———	80°	———	———	80°
M. S.	M. S.	0	M. S.	M. S.	0
1 2	0 54	70	0 33	0 33	70
0 58	1 2	60	0 39	0 34	60
1 17	1 18	50	0 44	0 44	50
1 46	1 37	40	0 55	0 55	40
2 5	2 16	30	1 17	1 18	30
3 14	3 10	20	1 57	1 57	20
5 42	5 59	10	3 44	3 40	10
Not obſerved.	Not obſerved.	0	40 10	Not obſerved.	0
Total time of cooling from 80° to 10°.			Total time of cooling from 80° to 10°.		
	M. S.			M. S.	
In Exp. N° 7. =	16 4		In Exp. N° 9. =	9 49	
In Exp. N° 8. =	16 16		In Exp. N° 10. =	9 41	
Medium =	16 10		Medium =	9 45	

By theſe Experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{45}{60}$ to $16\frac{10}{60}$ inverſely, or as 1000 to 603.

To determine whether the ſame law would hold good when the heated thermometers, inſtead of being plunged into freezing water, were ſuffered to cool in the open air, I made the following Experiments. The thermometers N° 1 and N° 2 being again heated in boiling water, as in the laſt Experiments, I took them out of the water, and ſuſpended them in the middle of a large room, where the air (which appeared to be perfectly at reſt, the windows and doors being all ſhut) was warm to the 16th degree of REAUMUR's thermometer, and the times of cooling were obſerved as follows:

(Exp. N° 11.) *Thermometer* N° 1. Surrounded by a Torricellian vacuum. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.		(Exp. N° 12.) *Thermometer* N° 2. Surrounded by air. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
——	80°	——	80°
M. S.	°	M. S.	°
Not obſerved.	70	Not obſerved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be nearly the ſame as in the foregoing Experiments, being as $6\frac{11}{60}$ to $10\frac{12}{60}$ inverſely, or as 1000 to 605. I could not obſerve the time of cooling from 80° to 70°, being at that time buſied in ſuſpending the inſtruments.

As it might poſſibly be objected to the concluſions drawn from theſe Experiments that, notwithſtanding all the care that was taken in the conſtructing of the two inſtruments made uſe of that they ſhould be perfectly alike, yet they might in reality be ſo far different either in ſhape or ſize, as to occaſion a very ſenſible error in the reſult of the Experiments; to remove theſe doubts I made the following Experiments:

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer ſtanding at 27 inches 11 lines, REAUMUR's thermometer at 15°, and the hygrometer at 47°, I repeated the Experiment N° 3. (of heating the thermometer N° 1 in boiling water, &c.) and immediately afterwards opened the cylinder containing the thermometer at its upper end, where it had been ſealed, and letting the air into it, I re-ſealed it hermetically, and repeated the Experiment again with the ſame inſtrument, the thermometer being now ſurrounded with air, like the thermometer N° 2.

The reſult of theſe Experiments, which may be ſeen in the following Table, ſhews evidently, that the error ariſing from the difference of the ſhapes or dimenſions of the two inſtruments in queſtion was inconſiderable, if not totally imperceptible.

(Exp. N° 13.)

Thermometer N° 1.

Its bulb half an inch in diameter ſhut up in the centre of a glaſs globe 1' inch in diameter, *voided of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.	Heat acquired.
	0°
M. S.	0
0 55	10
0 55	20
1 7	30
1 15	40
1 29	50
2 2	60
3 21	70
13 44	80

24 48 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 11′ 4″.

(Exp. N° 14.)

The ſame Thermometer (N° 1.)

The glaſs globe, containing the bulb of the thermometer, being now *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.	Heat acquired.
	0°
M. S.	0
0 32	10
0 32	20
0 43	30
0 50	40
1 1	50
1 24	60
2 38	70
10 25	80

18 5 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 7′ 40″.

It appears, therefore, from theſe Experiments, that the conducting power of common atmoſpheric air is to that of the Torricellian vacuum as $7\frac{40}{60}$ to $11\frac{4}{60}$ inverſely, or as 1000 to 602; which differs but very little from the reſult of all the foregoing Experiments.

Notwithſtanding that it appeared, from the reſult of theſe laſt Experiments, that any difference there might poſſibly have been in the forms or dimenſions of the inſtruments N° 1 and N° 2 could hardly have produced any ſenſible error in the reſult of the Experiments in queſtion; I was willing, however, to ſee how far any conſiderable alterations of ſize in the inſtrument would affect the Experiment: I therefore provided myſelf with another inſtrument which I ſhall call *Thermometer* N° 3. different from thoſe already deſcribed in ſize, and a little different in its conſtruction.

The bulb of the thermometer was of the ſame form and ſize as in the inſtruments N° 1 and N° 2. that is to ſay, it was globular, and half an inch in diameter; but the glaſs globe, in the centre of which it was confined, was much larger, being 3 inches 7½ lines in diameter; and the bore of the tube of the thermometer was much finer, and conſequently its length, and the diviſions of its ſcale, were greater. The diviſions were marked upon the tube with threads of ſilk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned inſtruments. The tube or cylinder belonging to the glaſs globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the centre of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a ſtopple of dry wood, covered with a coating of hard varniſh, through the centre or axis of which paſſed the end of the tube of the thermometer: this ſtopple confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a ſmall ſteel ſpring, a little below the point 0°; which, being faſtened to the tube of the thermometer, had three elaſtic points projecting outwards, which, preſſing againſt the inſide of the cylinder, confined the thermometer in its place. The total length of this inſtrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; conſequently this point lay about 1½ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the centre of its bulb coinciding with the centre of the globe. Through the ſtopple which cloſed the end of the cylinder paſſed two ſmall glaſs tubes, about a line in diameter, which being about a line longer than the ſtopple were cloſed occaſionally with ſmall ſtopples fitted to their bores. Theſe tubes (which were fitted exactly in the holes bored in the great ſtopple of the cylinder to receive them, and fixed in their places with cement) ſerved to convey air, or any other fluid, into the glaſs ball, without its being neceſſary to remove the ſtopple cloſing the end of the cylinder; which ſtopple, in order to prevent the poſition of the thermometer from being eaſily deranged, was cemented in its place.

I have been the more particular in the deſcription of theſe inſtruments, as I conceive it to be abſolutely neceſſary to have a perfect idea of them in order to judge of the Experiments made with them, and of their reſults.

With the inſtrument laſt deſcribed (which I have called *Thermometer* N° 3.) I made the following

Experiment. It was upon the 18th of July 1785, in the afternoon, the weather variable, alternate clouds and ſun-ſhine; wind ſtrong at S. E. with now and then a ſprinkling of rain; barometer at 27" inches 10½ lines, thermometer at 18°, and hygrometer variable from 44° to extreme moiſture.

In order to compare the reſult of the Experiment made with this inſtrument with thoſe made with the thermometer N° 2. I have placed together in the ſame Table the different Experiments made with them.

(Exp. Nº 15.) *Thermometer* Nº 3. Its bulb half an inch in diameter, ſhut up in the centre of a glaſs tube, 3 inches 7½ lines in diameter, and ſurrounded by *air*. *Taken out of freezing water, and plunged into boiling water.*		(Exp. Nº 4 and Nº 5.) *Thermometer* Nº 2 Its bulb half an inch in diameter, ſhut up in the centre of a glaſs globe, 1½ inch in diameter, and ſurrounded by *air*. *Taken out of freezing water, and plunged into boiling water.*			
		Time elapſed.			Heat acquired.
Time elapſed.	Heat acquired.	Exp. Nº 4.	Exp. Nº 5.	Medium.	
	0°				0°
M. S.	0	M. S.	M. S.	M. S.	0
0 33	10	0 30	0 30	0 30	10
0 38	20	0 35	0 37	0 36	20
0 54	30	0 41	0 41	0 41	30
0 51	40	0 49	0 53	0 51	40
1 7	50	1 1	0 59	1 0	50
1 28	60	1 24	1 20	1 22	60
2 28	70	2 45	2 25	2 35	70
9 0	80	9 10	9 38	9 24	80
16 59 = total time of heating from 0° to 80°.		16 55	17 3	16 59 = total time of heating from 0° to 80°.	
Time from 0° to 70° = 7′ 59″.		Time from 0° to 70° = 7′ 35″.			

If the agreement of theſe Experiments with the thermometers N° 2 and N° 3 ſurpriſed me, I was not leſs ſurpriſed with their diſagreement in the Experiment which follows:

Experi-

Experiment, N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately ſuſpended it in the middle of a large room, where the air, which was quiet, was at the temperature of 18°½ R. and obſerved the times of cooling as follows:

Time elapſed.	Heat loſt.
——	80°
M. S.	°
1 55	70
0 12	60
0 33	50
2 15	40
4 0	30
——	

9 55 = total time of cooling from 80° to 30°.

Time from 70° to 30° = 8′ 0″; but in the Experiment No. 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only 6′ 11″. In this Experiment, with the thermometer N° 3. the time employed in cooling from 60° to 30° was 7′ 48″; but in the above-mentioned Experiment, with the thermometer N° 2. it was only 5′ 20″. It is true, the air of the room was ſomewhat cooler when the former Experiment was made, than when this latter was made, with the thermometer N° 3; but this difference of temperature, which was only 2°½, (in the former caſe the thermometer in the room ſtanding at 16°, and in the latter at 18°½.) certainly could not have occaſioned the whole of the apparent difference in the reſults of the Experiments.

Does air receive Heat more readily than it parts with it? This is a queſtion highly deſerving of further inveſtigation, and I hope to be able to give it a full examination in the courſe of my projected inquiries; but leaving it for the preſent, I ſhall proceed to give an account of the Experiments which I have already made. Conceiving it to be a ſtep of conſiderable importance towards coming at a further knowledge of the nature of Heat, to aſcertain, by indiſputable evidence, its paſſage through the Torricellian vacuum, and to determine, with as much preciſion as poſſible, the law of its motions in that medium; and being apprehenſive that doubts might ariſe with reſpect to the Experiments before deſcribed, on account of the contact of the tubes of the incloſed thermometers in the inſtruments made uſe of with the containing glaſs globes, or rather with their cylinders: by means of which (it might be ſuſpected) that a certain quantity, if not all the Heat acquired, might poſſibly be communicated; to put this matter beyond all doubt, I made the following Experiment.

In the middle of a glaſs body, of a pear-like form, about 8 inches long, and 2½ inches in its greateſt diameter, I ſuſpended a ſmall mercurial thermometer, 5½ inches long, by a fine thread of ſilk, in ſuch a manner that neither the bulb of the thermometer, nor its tube, touched the containing glaſs body in any part. The tube of the thermometer was graduated, and marked with fine threads of ſilk of different colours, bound round it, as in the thermometers belonging to the other inſtruments already deſcribed; and the thermometer was ſuſpended in its place by means of a ſmall ſteel ſpring, to which the end of the thread of ſilk which held the thermometer being attached, it (the ſpring) was forced into a ſmall globular protuberance or cavity, blown in the upper extremity of the glaſs body, about half an inch in diameter; where the ſpring remaining, the thermometer neceſſarily remained ſuſpended in the axis of the glaſs body. There was an opening at the bottom of the glaſs body, through which the thermometer was introduced; and a barometrical tube being ſoldered to this opening, the inſide of the glaſs body was voided of air by means of mercury; and this opening being afterwards ſealed hermetically, and the barometrical tube being taken away, the thermometer was left ſuſpended in a Torricellian vacuum.

In this inſtrument, as the incloſed thermometer did not touch the containing glaſs body in any part, on the contrary, being diſtant from its internal ſurface an inch or more in every part, it is clear, that whatever Heat paſſed *into* or *out of* the thermometer muſt have paſſed *through* the ſurrounding Torricellian vacuum: for it cannot be ſuppoſed, that the fine thread of ſilk, by which the thermometer was ſuſpended, was capable of conducting any Heat at all, or at leaſt any ſenſible quantity. I therefore flattered myſelf with hopes of being able, with the aſſiſtance of this inſtrument, to determine poſitively with regard to the paſſage of Heat in the Torricellian vacuum: and this, I think, I have done, notwithſtanding an unfortunate accident that put it out of my power to purſue the Experiments ſo far as I intended.

This inſtrument being fitted to a ſmall ſtand or foot of wood, in ſuch a manner that the glaſs body remained in a perpendicular ſituation, I placed it in my room, by the ſide of another incloſed thermometer (N° 2.), which was ſurrounded by air, and obſerved the effects produced on it by the variation of Heat in the atmoſphere. I ſoon diſcovered, by the motion of the mercury in the incloſed thermometer, that the Heat paſſed through the Torricellian vacuum; but it appeared plainly from the ſluggiſhneſs, or great inſenſibility of the thermometer, that the Heat paſſed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I obſerved that the mercury in the thermometer ſurrounded by air deſcended much faſter than that in the thermometer ſurrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a veſſel of hot water (having no conveniencies at hand to repeat the Experiment in due form with the freezing and with the boiling water); and the thermometer ſurrounded by the Torricellian vacuum appeared ſtill to be much more inſenſible or ſluggiſh than that ſurrounded by air.

Theſe trials were quite ſufficient to convince me of the paſſage of Heat in the Torricellian vacuum, and alſo of the greater difficulty of its paſſage in that medium than in common air; but, not ſatisfied to reſt my inquiries here; I took the firſt opportunity that offered, and ſet myſelf to repeat the Experiments which I had before made with the inſtruments N° 1 and N° 2. I plunged this inſtrument into a mixture of pounded ice and water, where I let it remain till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of this cold mixture, I plunged it ſuddenly into a veſſel of boiling water, and prepared myſelf to obſerve the aſcent of the mercury in the incloſed thermometer, as in the foregoing Experiments; but unfortunately the moment the end of the glaſs body touched the boiling water, it cracked with the Heat at the point where it had been hermetically ſealed, and the water ruſhing into the body, ſpoiled the Experiment: and I have not ſince had an opportunity of providing myſelf with another inſtrument to repeat it.

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gaſſes, the thermometer N° 3. was conſtructed with a view to thoſe Experiments; and having now provided myſelf with a ſtock of thoſe different kinds of airs, I began with *fixed air*, with which, by means of water, I filled the globe and cylinder containing the thermometer; and ſtopping up the two holes in the great ſtopple cloſing the end of the cylinder, I expoſed the inſtrument in freezing water till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it into a large veſſel of boiling water, and prepared myſelf to obſerve the times of heating, as in the former caſes; but an accident happened, which ſuddenly put a ſtop to the Experiment. Immediately upon plunging the inſtrument into the boiling water, the mercury began to riſe in the thermometer with ſuch uncommon celerity, that it had paſſed the firſt diviſion upon the tube (which marked the 10th degree, according to REAUMUR'S ſcale) before I was aware of its being yet in motion; and having thus miſſed the opportunity of obſerving the time elapſed when the mercury arrived at that point, I was preparing to obſerve its paſſage of the next, when all of a ſudden the ſtopple cloſing the end of the cylinder was blown up the chimney with a great exploſion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and deſtroyed in its fall.

This unfortunate Experiment, though it put a ſtop for the time to the inquiries propoſed, opened the way to other reſearches not leſs intereſting. Suſpecting that the exploſion was occaſioned by the rarefaction of the water which remained attached to the inſide of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity with which the mercury roſe in the thermometer was principally owing to the ſame cauſe, I was led to examine the conducting power of *moiſt air*, or air ſaturated with water.

For this Experiment I provided myſelf with a new thermometer N° 4. the bulb of which, being of the ſame form as thoſe already deſcribed (*viz.* globular) was alſo of the ſame ſize, or half an inch in diameter. To receive this thermometer a glaſs cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the centre of this globe the bulb of the thermometer was confined, by means of the ſtopple which cloſed the end of the cylinder; which ſtopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its centre or axis, and confined the thermometer in its place, without the aſſiſtance of any other apparatus. Through this ſtopple two other ſmall holes were bored, and lined with thin glaſs tubes, as in the thermometer N° 3. opening a paſſage into the cylinder, which holes were occaſionally ſtopped up with ſtopples of cork; but to prevent accidents, ſuch as I have before experienced from an exploſion, great care was taken not to preſs theſe ſtopples into their places with any conſiderable force, that they might the more eaſily be blown out by any conſiderable effort of the confined air, or vapour.

Though in this inſtrument the thermometer was not altogether ſo ſteady in its place as in the thermometers N° 1, N° 2, and N° 3. the elaſticity of the tube, and the weight of the mercury in the bulb of the thermometer, occaſioning a ſmall vibration or trembling of the thermometer upon any ſudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being entirely free, or ſuſpended in ſuch a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of Heat received by the tube of the thermometer at its contact with the cylinder at its choaks, in the inſtruments N° 1 and N° 2. or with the branches of the ſteel ſpring in N° 3. and from thence communicated to the bulb, muſt have been exceedingly ſmall; yet I was deſirous to prevent even that, and every other poſſible cauſe of error or inaccuracy.

Does humidity augment the conducting-power of air?

To determine this queſtion I made the following Experiments, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

<table>
<tr><th colspan="2">(Exp. Nº 17.)
Thermometer Nº 4.
Surrounded by air *dry* to the 44th degree of the quill hygrometer of the Manheim Academy.
Taken out of freezing water, and plunged into boiling water.</th><th colspan="2">(Exp. Nº 18.)
The ſame Thermometer (Nº 4.)
Surrounded by air rendered as *moiſt* as poſſible by wetting the inſide of the cylinder and globe with water.
Taken out of freezing water, and plunged into boiling water.</th></tr>
<tr><td>Time elapſed.</td><td>Heat acquired.</td><td>Time elapſed.</td><td>Heat acquired.</td></tr>
<tr><td>———</td><td>80°</td><td>———</td><td>0°</td></tr>
<tr><td>M. S.</td><td>°</td><td>M. S.</td><td>°</td></tr>
<tr><td>0 34</td><td>10</td><td>0 6</td><td>10</td></tr>
<tr><td>0 39</td><td>20</td><td>0 4</td><td>20</td></tr>
<tr><td>0 44</td><td>30</td><td>0 5</td><td>30</td></tr>
<tr><td>0 51</td><td>40</td><td>0 9</td><td>40</td></tr>
<tr><td>1 6</td><td>50</td><td>0 18</td><td>50</td></tr>
<tr><td>1 35</td><td>60</td><td>0 26</td><td>60</td></tr>
<tr><td>2 40</td><td>70</td><td>0 43</td><td>70</td></tr>
<tr><td>not obſerved.</td><td>80</td><td>7 45</td><td>80</td></tr>
<tr><td colspan="2">8 9 = total time of heating from 0° to 70°.</td><td colspan="2">1 51 = total time of heating from 0° to 70°.</td></tr>
</table>

From theſe Experiments it appears, that the conducting power of air is very much increaſed by humidity. To ſee if the ſame reſult would obtain when the Experiment was reverſed, I now took the thermometer with the *moiſt air* out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I obſerved the times of cooling, as ſet down in the following Table. N. B. To compare the reſult of this Experiment with thoſe made with *dry air*, I have placed on one ſide in the following Table the Experiment in queſtion, and on the other ſide the Experiment Nº 19. made with the thermometer Nº 2.

(Exp. N° 19.)
Thermometer N° 4.
Surrounded by *moiſt air.*
Taken out of boiling water, and plunged into freezing water.

Time elapſed.	Heat loſt.
——	80°
M. S.	°
0 4	70
0 14	60
0 31	50
0 52	40
1 22	30
2 3	20
4 2	10

9 8 = total time of cooling from 80° to 10°.

(Exp. N° 10.)
Thermometer N° 2.
Surrounded by *dry air.*
Taken out of boiling water, and plunged into freezing water.

Time elapſed.	Heat loſt.
——	80°
M. S.	°
0 33	70
0 34	60
0 44	50
0 55	40
1 18	30
1 57	20
3 40	10

9 12 = total time of cooling from 80° to 10°.

Though the difference of the whole times of cooling from 80° to 10° in theſe two Experiments appears to have been very ſmall, yet the difference of the times taken up by the firſt twenty or thirty degrees from the boiling point is very remarkable, and ſhows with how much greater facility Heat paſſes in moiſt air than in dry air. Even the ſlowneſs with which the mercury in the thermometer N° 4. deſcended in this Experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in ſome meaſure to the great conducting power of the moiſt air with which it was ſurrounded; for the cylinder containing the thermometer and the moiſt air, being not wholly ſubmerged in the freezing water, that part of it which remained out of the water was neceſſarily ſurrounded by the air of the atmoſphere; which being much warmer than the water, communicated of its Heat to the glaſs; which, paſſing from thence into the contained moiſt air as ſoon as that air became colder than the external air, was, through that medium, communicated to the bulb of the incloſed thermometer, which prevented its cooling ſo faſt as it would otherwiſe have done. But when the weather becomes cold, I propoſe to repeat this Experiment with variations, in ſuch a manner as to put the matter beyond all doubt. In the mean time I cannot help obſerving, with what infinite wiſdom and goodneſs Divine Providence appears to have guarded us againſt the evil effects of exceſſive Heat and Cold in the atmoſphere; for if it were poſſible for the air to be equally damp during the ſevere cold of the winter months as it ſometimes is in ſummer, its conducting power, and conſequently its apparent coldneſs, when applied to our bodies, would be ſo much increaſed, by ſuch an additional degree of moiſture, that it would become quite intolerable; but, happily for us, its power to hold water in ſolution is diminiſhed, and with it its power to rob us of our animal heat, in proportion as its coldneſs is increaſed. Every body knows how very diſagreeable a very moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a juſt meaſure of the apparent or ſenſible Heat of the atmoſphere. If colds or catarrhs are occaſioned by our bodies being robbed of our animal heat, the reaſon is plain why thoſe diſorders prevail moſt during the cold autumnal rains, and upon the breaking up of the froſt in the ſpring. It is likewiſe plain from whence it is that ſleeping in damp beds, and inhabiting damp houſes, is ſo very dangerous; and why the evening air is ſo pernicious in ſummer and in autumn, and why it is not ſo during the hard froſts of winter. It has puzzled many very able philoſophers and phyſicians to account for the manner in which the extraordinary degree or rather *quantity* of Heat is generated which an animal body is ſuppoſed to loſe, when expoſed to the cold of winter, above what it communicates to the ſurrounding atmoſphere in warm ſummer weather; but is it not more than probable, that the difference of the quantities of Heat, actually loſt or communicated, is infinitely leſs than what they have imagined? Theſe inquiries are certainly very intereſting; and they are undoubtedly within the reach of well contrived and well conducted Experiments. But taking my leave for the preſent of this curious ſubject of inveſtigation, I haſten to the ſequel of my Experiments.

Finding ſo great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of denſity. For this Experiment I prepared the thermometer N° 4. by ſtopping up one of the ſmall glaſs tubes paſſing through the ſtopple, and opening a paſſage into the cylinder, and by fitting a valve to the external overture of the other. The inſtrument, thus prepared, being put under the receiver of an air-pump, the air paſſed freely out of the globe and cylinder upon working the machine, but the valve above deſcribed prevented its return upon letting air into the receiver. The gage of the air-pump ſhowed the degree of rarity of the air under the receiver, and conſequently of that filling the globe and cylinder, and immediately ſurrounding the thermometer.

With this inſtrument, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following Experiments.

(Exp. N° [illegible])		(Exp. N° 21.)		(Exp. N° 22.)	
Thermometer N° 4. Surrounded by common air, barometer ſtanding at 27 inches 9 lines. *Taken out of freezing water, and plunged into boiling water.*		*Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 6 inches 11½ lines. *Taken out of freezing water, and plunged into boiling water.*		*Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 1 inch 2 lines. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°		0°
M. S.	0	M. S.	0	M. S.	0
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not obſerved.	80
7 36 = total time of heating from 0° to 70°.		7 37 = total time of heating from 0° to 70°.		7 51 = total time of heating from 0° to 70°.	

The reſult of theſe Experiments, I confeſs, ſurpriſed me not a little; but the diſcovery of truth